1925, 1935, 1897, 1928, 1937, 1922, 1937, 1922, 1925, 1927, 1930, 1927, 1911, 1923, 1897, 1921, 1922, 1923.

With Hadamard the editors have grouped his papers into 14 (often historically and mathematically inseparable) sections, within which they do appear chronologically. With neither Borel nor Hadamard, however, does the date of publication appear with the paper. Hence, to discover when a particular paper was written - given no clues within the text itself - one must resort to the chronologically ordered complete list of publications and check through them one by one! In Riesz' case, the editors have been more thoughtful, including at the beginning of each of the nine sections full bibliographic information regarding the papers appearing in that section. On the other hand, it is with Riesz that the concept of compartmentalisation reaches its peak. Here one finds such groupings as Theorie der reellen Funktionen, Funktionräume (both in volume I), and Funktionanalysis (in volume II). To the scholar delving into the origins of functional analysis, such an artificial classification is nothing short of an absurdity.

One may well ask for whom were the works (of, say, Borel and Hadamard) collected? For the research mathematician? Unlikely, as she/he rarely shows interest in "the classics" - i.e., a paper over 25 years old. For the historian of mathematics then? Possibly, but if so the collectors have done a remarkably poor job. No doubt the editors/publishers had no clear audience in mind, and perhaps were pursuing their botanical hobby for the greater glory of French mathematics. (If this is the case, where are the *Oeuvres* of Liouville and Poisson?) I conclude therefore with a plea, that historians of mathematics take a hand in editing future collected works in order to make them more useful to both mathematicians and historians.

ERNST ZERMELO, A. E. HARWARD, AND THE AXIOMATIZATION OF SET THEORY

A Note from Gregory H. Moore, University of Toronto

In 1908 Ernst Zermelo gave the first axiomatization of set theory. However, many of Zermelo's ideas appeared previously in a paper by an obscure British mathematician, A. E. Harward [1905]. I do not imply that Harward influenced Zermelo or anyone else, and I am unconcerned with Harward as a precursor of Zermelo. Rather, Harward's paper raises the question how Zermelo's ideas were related to Harward's, and why the latter's paper elicited no response. A tantalizing question, which I shall not attempt to answer here, is the sense in which certain of Zermelo's ideas were "in the air".

Zermelo [1908] proposed seven axioms, which I summarize for later comparison with Harward [1905]: Axiom of Extensionality (two sets with the same elements are identical); Axiom of Elementary Sets (there exists a set with no elements, and for any sets or individuals b and c, there exist the sets $\{b\}$ and $\{b,c\}$); Axiom of Separation (for any set A there is a subset B containing exactly those elements x of A satisfying a *definit* propositional function P(x)); Power set Axiom (for any set A the set of all subsets of A exists); Union Axiom (for any set A there is a set containing exactly the members of members of A); Axiom of Choice (for any family T of disjoint non-empty sets there is a subset S of the union of T such that S has exactly one element in common with each member of T); Axiom of Infinity (there is a set Z containing the empty set and containing $\{A\}$ if it contains A).

Employed in Calcutta by the Indian Civil Service, Harward had written his paper in December 1904. Then he submitted it to Philip Jourdain, who corrected some errors [Harward 1905, 439]. In particular, the paper was stimulated by weaknesses in Jourdain's [1904, 1904a] proof of the Schröder-Bernstein Theorem and of the theorem that K = (aleph-null) $\cdot K = K^2$ for any transfinite cardinal K. More generally, Harward attempted to develop rigorously all of transfinite cardinal arithmetic.

In order to avoid Burali-Forti's paradox, Jourdain [1904, 66] had distinguished between "consistent" and "inconsistent aggregates". Formally, an aggregate A was inconsistent if the aggregate of all ordinals could be mapped one-one into A. Informally, an inconsistent aggregate was one which could not be considered as a whole without contradiction, e.g., the aggregate of all ordinals. Preserving Jourdain's distinction, Harward changed his terminology to "limited" and "unlimited classes". Unlike Jourdain, Harward restricted the term aggregate to limited classes.

Although Harward's [1905] approach was not strictly axiomatic, several of his statements were in essence Zermelo's later axioms. From his definitions, Harward concluded that every subclass of an aggregate is an aggregate [p.440], an assertion similar to the Axiom of Separation. He required that the class of subclasses of an aggregate be an aggregate and that the union of an aggregate of aggregates be an aggregate [pp.440, 441] - analogous to the Union Axiom and the Power Set Axiom. Moreover, Harward assumed that a denumerable class was an aggregate [p.443], a statement suggestive of the Axiom of Infinity.

Three of Zermelo's axioms were not suggested by Harward. There was no Axiom of Extensionality, and no Axiom of Elementary Sets. The Axiom of Choice was more problematical, for

Harward stated:

> *The multiplicative class of an aggregate of aggregates, no two of which have any common element, is the class each of whose terms is an aggregate formed by taking one and only one element from each of those aggregates. I provisionally assume as an axiom that the multiplicative class of an aggregate of aggregates is itself an aggregate. The necessity for assuming some axiom arises from the informal character of our definition of the term aggregate, ...* [p.441]

At first this seems to state the Axiom of Choice, in the form of the Multiplicative Axiom found later in Russell [1906]. But Harward merely excluded the possibility that a multiplicative class was an unlimited class. He did not explicitly require such a multiplicative class of an aggregate of non-empty aggregates to be *non-empty*, a demand equivalent to the Axiom of Choice. This interpretation is reasonable since Harward, following Jourdain [1904], employed an infinity of successive, dependent choices to show that any aggregate can be well-ordered and did not explicitly use the Axiom of Choice in the proof [Harward 1905, 444].

However, Harward went further than Zermelo in two respects. First, like Jourdain, Harward allowed classes which were too large to be sets. Thus both men were close to the later distinction between sets and proper classes. Second, Harward made the surprising statement that: *Any class of which the individuals can be correlated one to one with the elements of an aggregate is itself an aggregate."* [p. 440] This is the crux of Abraham Fraenkel's and Thoralf Skolem's Axiom of Replacement, which appeared in 1922 and supplemented Zermelo's axioms.

Harward [1905] contained the core of what could have been a worthwhile axiomatization of set theory. Nevertheless, his paper provoked no public response, even from Jourdain who had suggested changes in it prior to publication. This silence was partly due to the fact that Harward was a self-confessed amateur vis-à-vis set theory [p.439]. In addition, he himself was not satisfied with his distinction between aggregates and unlimited classes, for there were certain large classes which he regarded as aggregates but which he was unable to prove so. However, he did not state what large classes he had in mind. Another reason for lack of response to his paper as an axiomatization of set theory, instead of a definition of set, is apparent in his conclusion: *In a paper of later date, I have discussed the logical relations between the various axioms* [for aggregates] *which we may assume, and have arrived at a formal definition* [of aggregate] *which appears to me to be satisfactory and which obviates the necessity for assuming any axiom.* [p.459] Thus Harward's approach was basically anti-axiomatic. Regrettably, that "paper of later date" never appeared, although it is unlikely that his definition would have been successful.

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I have been unable to discover anything further about Harward's life and do not even know what his full name is. I have found only one other paper by him [1922]. Any information on his life or mathematical interests would be received appreciatively and should be forwarded to me care of the Editorial Office of *Historia Mathematica*.

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D. PINGREE'S REVIEW OF MY BOOK: SCIENCE AWAKENING II

By B. L. van der Waerden, Zurich

In this rejoinder, I shall restrict myself to those sentences in Pingree's review that are wrong statements or unproved conjectures. The most disagreeable wrong statement is the last one: "This book will probably be used in classrooms in American colleges. Unfortunately the students who are asked to read it will in general have no way of distinguishing what is plausible as history and what is not." This is not true, for I took great pains throughout the book never to present conjectures as facts and to give all the arguments underlying my conclusions in full, so that any reader can judge their value. Moreover, most of my conclusions are based on genuine texts from ancient authors such as Darius, Zarathustra, Plato, etc., and in the astronomical part, on cuneiform texts.

On page 91 Pingree writes: "[The author] believes that laws of symmetry and simplicity are as applicable to history as they